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## THE INTERLANGUAGE NOTION<sup>1</sup>

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It is generally acknowledged nowadays that the nature of the linguistic systems constructed by second language learners as they learn—or even after they have stopped learning—is a fruitful area for applied linguistic research. It has been suggested at various times that the findings of this sort of research will be useful not only to language teachers and textbook writers, but also to the development of a general theory of language acquisition. In this paper I will examine some of the claims for and about this area of research that have been made over the years since public attention was first focused on it by Selinker (10).

We used to believe that we would be able to predict some of the behavior of second language (L<sub>2</sub>) learners by looking at the similarities and differences between their native language (NL) and the target language (TL) that they were trying to learn. We thought that if we did a rigorous enough analysis of the NL and the TL and then compared the two analyses, we would have the equipment we needed to prepare the best pedagogical materials possible. The contrastive analysis would tell us where the two languages differed, and thus where we could expect interference (now called language transfer or interlingual error, among other terms) to crop up; it would also tell us where the two languages were similar, and thus where we would be able to expect facilitation of language learning. Making up our textbooks and lesson plans, we would naturally place more emphasis on areas of interference, and less on areas of facilitation. The “strong contrastive analysis hypothesis” (6) predicted that *all* errors made by L<sub>2</sub> learners would be interference errors. This was easily disproved when researchers began to make a serious effort to catalogue and classify learners’ errors: many errors appeared which had no discernible source in the NL. A weaker version of the hypothesis was then advanced (12), predicting that *some* errors would be predicted by contrastive analysis. Evidence to refute hypotheses containing the quantifier ‘some’ is virtually impossible to find, but it is surprising how little research has apparently been done to pin down what sorts of errors are based on interference and what sorts are not. Most of us believe that pronunciation problems are usually caused by language transfer (although there is evidence that the actual speech of learners may be influenced by their NL in more sophisticated ways than we used to think—cf. 4); and most of us believe that interference errors diminish and are replaced by other types of error as learners become more proficient. An experiment with Thai learners (8) bore this out with an item analysis of a discrete-point test, in which one distractor was based on Thai and another on English; the more proficient students, as measured by their scores on the TOEFL, more often picked either the English-based distractor or the correct response, and the less proficient students more often picked the Thai-based distractor.<sup>2</sup>

The current focus on learners’ overall linguistic systems promises to lead researchers to look at questions such as this. It has been argued that correct performance can be just as revealing as incorrect performance, and that the linguistic competence of L<sub>2</sub> learners can and should be

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<sup>1</sup> An earlier version of this paper was given at the 1982 Mid-America Linguistics Conference and appears in Ingemann (5).

<sup>2</sup> Aside from phonological problems, other kinds of persistent interference include certain semantic categories overtly represented in the TL but not in the NL. Among these are grammatical gender, which is very difficult for English speakers learning languages in which nouns have gender, and is more difficult for Turkish or Persian speakers learning English with its minimal gender-marking in the pronoun system. Other resistant categories appear to include dimensions such as *narrow vs. tight* or *high vs. tall*, which are not overtly represented in many languages.

described (7, 9, 3). The name given to this competence by Selinker (10) is Interlanguage (IL).

Although Selinker did not, in his original article, explicitly state what has now come to be known as “the interlanguage hypothesis,” he laid the groundwork for a number of studies based on this notion. One study (11) succinctly stated the hypothesis as follows (p. 96):

There exists a separate linguistic or psycholinguistic system (interlanguage) which forms in the mind of the learner and may take the form of a pidgin and which may develop into a language in its own right.

Tarone et al. go on to detail the IL hypothesis (p. 97):

There are four sets of observable facts upon which the IL hypothesis is based, and which may be used to evaluate that hypothesis. Each of these observable facts is studyable: first, the *stability* over time of certain errors and other surface forms in learner-language systems (i.e., “fossilization”); second, the *mutual intelligibility* that appears to exist among speakers of an IL; third, the phenomenon of *backsliding*, or the regular appearance in bilingual speech of errors that were thought to be eradicated; and fourth, the *systematicity* of the IL at one particular point in time. [Emphasis original.]

Since the introduction of IL to the applied linguistics lexicon, the term has been generally accepted to mean the language learner’s *use* of the TL, usually in speech, and always in communication. I believe that it is seldom, if ever, used to refer to TL production during language learning exercises such as drills.

Among the attributes we require of scientific hypotheses are that they account for phenomena not explained, or less elegantly explained, by previous hypotheses; and that they be falsifiable. That is, they must make predictions about observable events in a narrow enough way that should these predictions not be borne out, the hypothesis can be disproved. An assertion that makes no predictions, or whose predictions can never be disproved, is not a hypothesis; an irrefutable assertion explains nothing. If I insist that the universe rests on the back of a large turtle, my assertion cannot be tested because we cannot leave the universe and take a look to see if what is holding it up is in fact a turtle. It might be an elephant; for all we know, there might be elephants all the way down. Since the assertion cannot be tested or disproved by observation of the facts, it is not a hypothesis and tells us nothing about the nature of the universe.

Turning to narrower data, such as language, we can also make refutable and irrefutable assertions. If I claim that no language will be found with voiced fricatives but no voiceless ones, my claim is easily disproved, since someone could find a counterexample tomorrow in a language which did have only voiced fricatives. My assertion is thus a hypothesis, since (however crudely put) it gives a reason for our not having found such languages, and yet it could be falsified quite simply. But if I turn it around and say that I think there *are* languages with only voiced fricatives, my claim can never be disproved; I can always say we haven’t looked hard enough. The “weak contrastive analysis hypothesis” is this sort of claim, and I shall argue below that the IL hypothesis is of the same sort.

Before looking at the refutability of the IL hypothesis, let us consider whether it accounts for what previous claims have failed to deal with. Tarone et al. cite the four sets of observable facts listed earlier: *stability*, *mutual intelligibility*, *backsliding*, and *systematicity*. Are any of these explained, or accounted for in a new improved way, by the IL hypothesis? Let us examine each in turn.

*Stability.* By this workers in the IL field mean showing “consistency in the use of forms *over time*” (Tarone et al., p. 97; emphasis original). In other words, L<sub>2</sub> students who use the same forms the second time you look at them as they did when you first looked are showing stability. It is unclear to me that an entire new hypothesis about language is needed to explain the human propensity to learn things slowly, and to keep making the same mistakes. Such stability as is shown by L<sub>2</sub> learners in their use of the TL can be as well, if not better, explained by the more general notion that language is rule-governed behavior. The matter of stability becomes even less interesting when we discover that Tarone et al. have decided to distinguish two types of IL users. A type I individual is one whose IL is characterized by stability, and a type II individual is one whose IL is characterized by instability—in other words, type IIs continue to learn. Thus one of the facts that the IL hypothesis purports to explain turns out to be dealt with quite circularly. If your students’ ILs are stable, they must be type Is; if they are unstable, they must be type IIs. How do you tell which type a person is? By the stability of his/her IL.

*Systematicity.* Tarone et al. call L<sub>2</sub> speech systematic “when it evidences an internal consistency in the use of forms at a *single point* in time” (p. 97; emphasis original). It is not clear just how internally consistent L<sub>2</sub> speech has to be before it is considered systematic. It is not clear whether ILs are just as systematic as NLs, more so, or less so. In fact, the use of the term ‘systematic’ in this binary sense implies that somewhere there is a line on one side of which things are [+systematic] and on the other side of which they are [–systematic]. I believe, however, that the term is being used simply to mean ‘not random’, rather than ‘describable by rule’. In one sense, ILs can be said to be more systematic than NLs, in that they display less stylistic variation: I know that when I communicate in a foreign language, I use a much smaller range of styles than native speakers do, because I have a much smaller range at my command. I therefore have more systematicity, according to this definition, in my IL than in my NL. We may be faced with the type I and type II individuals again, classified into those types on the basis of the stability of their ILs.

*Mutual intelligibility.* Language teachers are fond of recounting anecdotes about a Japanese speaker interpreting the English of an Arabic speaker for the teachers, or a group of students apparently conversing while native English speakers listen uncomprehendingly. While more research is appearing on language use between L<sub>1</sub> and L<sub>2</sub> speakers, I know of no serious research being done on the mutual intelligibility question. Do the students from one class understand those from another, or only their classmates? Do more advanced students understand less advanced students, or is it the reverse? How can you tell? Adjemian puts the case admirably for having one’s mutual intelligibility cake and eating it (1976:300):

Mutual intelligibility is an inherent property of ILs as a result of their being members of the set of possible human languages. The notion “mutual intelligibility” is relevant at the very onset of a study: it must first be established whether a group of learners can communicate verbally with each other in a language other than their NL. If so, then it may be assumed that they share an IL. If not, then it may be claimed that they do not yet possess enough of a non-native grammar to have caused the emergence of an IL.

The mutual intelligibility that the IL hypothesis is supposed to explain is thus adduced as evidence for the IL itself. If they can understand each other, they have an IL—how else could they do it? If they can’t, they must not have one—if they did have one, they would be able to understand. This kind of reasoning can be applied to tennis: if you hold your mouth right, you will serve an ace. If you don’t serve an ace, you must have been holding your mouth wrong.

*Backsliding.* All language teachers are familiar with, and despair of, backsliding. The term is used to mean the apparent mastery of a linguistic form in the TL, followed by loss of the mastery and misuse or nonuse of the form. I do not see that backsliding is peculiar to L<sub>2</sub> learning, or that we need to explain it in terms other than the normal patterns of human learning behavior, whether one is trying to learn to ride a bicycle, play an instrument, or be fair to one's spouse. It is not, in other words, a linguistic phenomenon. In fact, the IL hypothesis seems to me to predict, by its insistence on systematicity and/or stability, more that backsliding would *not* occur than that it would.

None of the four phenomena that the IL hypothesis purports to account for, then, needs to be explained by a new linguistic hypothesis. Let us now look at the falsifiability of the entire hypothesis. Does it predict observable events? Could it be disconfirmed by looking at observable data? In order to disconfirm it, one would have to show that there is in fact no system separate from the NL and the TL which develops in the mind of the learner. One would have to demonstrate either (a) that the learner has simply grafted some TL forms onto the internalized NL system, or (b) that he/she has in fact internalized the TL system in its entirety but also has a set of rules which mess it up when he/she tries to use it. Neither of these positions is provable, nor could any data be found to support them.

There is nothing, therefore, that we can ever hope to observe which would disconfirm the IL hypothesis. It therefore tells us nothing about second language acquisition. It makes no prediction which could ever turn out not to be the case. What good is it, then? Is the emperor really wearing no clothes? I think that the *notion* of IL has been very useful in stimulating us to look beyond errors made by L<sub>2</sub> users, and to try to look at the whole of whatever linguistic systems they appear to be using. I believe that we are far more likely to be able to find a way to account for L<sub>2</sub> competence (or incompetence) by looking for systems, and that in getting us away from our preoccupation with errors, IL has done the field a service. If professionals in language teaching now want to use the term to mean "communicating in a foreign language", that's fine. But let's stop calling it a hypothesis and waiting for it to explain something.

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